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IMAGELESS THOUGHT: RÉSUMÉ AND CRITIQUE

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When Bühler published the conclusions which he saw fit to draw from his experimental analysis of the thought-process (12), he doubtless realized the storm of opposition which his views must meet. The storm has not yet abated, though it has taken a different turn from that of the first strong gust which issued from Wundt (48, 49) in his violent attack on the method which Bühler had used. In this respect, indeed, the cause appears to be making its most consistent progress. It is only necessary to survey the steadily increasing number of theses which have adopted the 'Würzburg Method' to be convinced that it has won recognition as a valuable addition to psychological procedure. As Michotte has well pointed out (26), if despite the incomplete character of the observations, one arrives at positive results, these results may be quite as valuable as any others. And at least with respect to the established importance of the 'determining tendencies' and 'tasks,' the method has justified its existence.

But the central contention of Bühler still remains an open question, and in America it appears to win adherents but slowly. Indeed, so strongly negative is the outcome of the investigations which have been undertaken in the laboratories of Cornell and Clark, where a large part of the recent work on this problem has been done, that it has seemed worth while, perhaps, for one who sympathizes with the 'imageless contents' to present a brief review and critique of some of the results thus far obtained.

By way of introduction, I may say a few words regarding the question of historical priority in this field. The credit which has

generally been accorded the Würzburg School in this innovation has been contested by Binet (7) who states that the 'method of Würzburg' were better named the 'method of Paris.' No one need doubt that Binet's results, published in 1903, were reached quite independently of the Würzburg work, but although it was not until 1907 that Bühler, in a sense, brought to a culmination the Würzburg investigations, Marbe had already used the method and published his results on the judgment in 1901 (24). Ach, too, who began his work in the Würzburg laboratory in 1900, claims the credit of priority not only for the method in its systematic form, but also for the 'determining tendency' and '*Bewusstheit*,' or imageless awareness of meaning (2, 18, 286, 9-10). The work of both Binet and the Würzburg people dates from the same year, 1900, but in Würzburg a more systematic method was employed from the start.

As for the results achieved, they had already been indicated in speculative literature considerably before this. James (19) in his *Principles* (1890) had advocated 'transitive parts' in the stream of thought, and he found in the writings of no less than five predecessors support for his belief in 'feelings of relation.' Similarly Stout (38) advocated the understanding of words without images through the mediation of 'psychic fringes' in 1896, while Ehrenfels' 'Gestalt-qualitäten' (1890) (16) also play a rôle in the speculative origin of similar views of thought.

But we are here dealing with experimental results only, and should next refer to the independent findings of Woodworth (44, 45, 46, 47). In his study of voluntary movement (1903) he first became aware of the fact that a voluntary movement was not always nor typically called out by a kinesthetic image of the movement to be made. Later he took up this problem of the antecedent image with a larger number of observers. He did not anticipate the cases of no image at all, he writes me, but only the lack of kinesthetic images. "As the work progressed it became evident that the subjects were often unable to detect any image whatever; and so," he says, "I was led to the conclusion that one could have the thought of a movement without any image of it."

The experimental investigations which first pointed to the existence of imageless conscious contents have, then, at least three independent sources: Würzburg, Paris and New York, and the question of strict historical priority is of less importance than the fact that the discovery should have been made independently in each of these three places. In this connection, however, it is interesting to note that

each pioneer approached the question with a different problem in mind. The Würzburg work is the logical outcome of Külpe's emphasis in 1893 on the part that the preparation or disposition plays in all reaction experiments (21). This was again stated by him in an article on Attention which appeared in 1902 (22), but was written in 1899. These considerations doubtless paved the way for the future discovery of the 'determining tendency' and the 'task.' Regarding the character of thought as a distinct mental element, he tells me that the idea first came to him in the summer of 1898 while lecturing on Leibniz, when it occurred to him that the 'monads' were not concepts but *thoughts*. Binet's work, in turn, was the natural consequence of years of individual question and answer tests which he had employed with all manner of subjects for the purpose of characterizing and classifying individual types. Woodworth, finally, hit upon his discovery in the course of a study of the movement consciousness, as we have already noted.

So much, then, for the essential facts in the history of the subject. We come next to an examination of the results achieved. The work of the Würzburg School has been so thoroughly exploited in review and criticism, that I shall content myself with a bare enumeration of its principal phases. To Marbe (24) we owe the suggestive term *Bewusstseinslage*, the exact content of which is still, however, a matter of uncertainty. Originally used as a mere negative description for states of consciousness which were certainly present, but which seemed to defy closer analysis, the term received a further negative definition from Orth (31) who differentiated it from feeling. With Messer (25) it assumes direct relationship with thought, but Ach, who at first intimated its proximity to one of the forms of *Bewusstheit* (1, 236), has more recently declared that the two are entirely disparate (2, 9), since Marbe has denied to the *Bewusstseinslage* that which is characteristic of *Bewusstheit*, namely, the imageless knowing quality. The significance most commonly assigned to it consists in its identification with conscious attitudes.

To Ach (1) and Watt (42) we owe the careful experimentation which resulted in the more precise description of the important and far-reaching influence of the problem at hand and the determining tendencies which issue from it, as had already been foreshadowed in Külpe's writings. Ach also introduces the first experimental findings of this school for the existence of imageless knowing contents. The *Bewusstheit* is accompanied by sensations or memory images which stand as conscious symbols for the knowing quality. It is,

however, not reducible to these factors, for they may be present without this knowing quality. Ach also gives us a theory to explain the *Bewusstheit*. Every idea, he says, sets a number of other ideas which are associatively connected with it, in readiness for reproduction. This readiness for reproduction suffices for the *Bewusstheit*, without it being at all necessary for the associated ideas to appear in consciousness. The *Bewusstheit* is thus described as a developing function which represents a state of excitation among the reproductive tendencies (1, 219).

Kakise (20) in his study of understanding, at Clark, has reached a similar conclusion. His introspective reports suggest six stages in the understanding process: (1) pure feeling of familiarity, (2) pure feeling of concept or meaning, (3) pure feeling of content, (4) feeling of direction, (5) half developed images, (6) fully developed images. The first of these stages, being founded on past experience, is described as a 'retrospective quality' attaching to the sensations of the stimulus. The second is either a further development of this, or is identical with the third stage. It is here that we meet with the 'imageless thought' in its unequivocal form, and the author concludes, evidently by reference to his further stages of image development, that this stage consists in incipient excitations of a number of past experiences or images. This does not, in his opinion, yield a new element of thought, for thought of this character is neither elemental "nor a highly elevated something, but merely the most primitive and rudimentary form of reproduction" (20, 62).

We must note in connection with this interpretation that, whereas Ach insists that a complete arousal of the reproductions in question is quite unnecessary, Kakise concludes that "whether or not one has in the understanding of a word or phrase a concrete representation depends primarily upon the duration, *i. e.*, upon the time one dwells upon it" (20, 64). Kakise used two methods, an 'active' and a 'passive.' The first is the usual reaction method. He gives us no tabulation of his time results, but we may perhaps gather that they would not average over 1" for word stimuli. Images were infrequent with this method. The 'passive' method, which he considers more effective, eliminates the reaction and makes the duration uniformly 3". The results with this method show a preponderance of imagery.

The question as to whether the presence of rudimentary representative processes may be deemed adequate to the consciousness of understanding is basic to this whole problem. The advocates of the imageless content say that it is not; the opposition, in turn, says that

it is. It has never been denied, so far as I know, that some sort of imagery may be present at all times. It may be partially incipient, as Kakise describes it, and partially vestigial as Book (9), and Washburn (41) have shown. But the writers who claim that this is all we can know, and all that we need to know, concerning the psychology of thought simply beg the whole question of the imageless content for the negative. Furthermore, the fact which Kakise adduces that increased duration is sufficient to bring the image clearly into focus, is, in my opinion, no argument against the imageless factors. With his 'passive' method he has simply made imagery the chief desideratum, as was also largely the case in Schwiete's (35) and Störing's (36, 37) investigations where the reaction times often run from 5"-20". It is precisely in the brief moments of active thinking that the thought factor is most apparent. Whenever the constraint of a relatively quick reaction is removed, a premium is placed on discursive thought accompanied by a plentiful supply of words and images.

Coming back to our Würzburg workers, it remained for Bühler to find thoughts independent of any sensory factors, feelings or attitudes, elements which possessed neither sensory quality nor intensity, but manifested a degree of clearness, assurance and vividness. Bühler contends that images are too fragmentary and sporadic to be essential to thought. Thoughts occur without images as well as without them, nor does the thought become less clear as the image fades. As for the attitudes, they are not *actual* enough to cover the case. The consciousness of relation is too narrow a category, because, first, all consciousness of relation is not directed on images, and second, we often know the *what* of the thought before any image has developed. Bühler also notes that the laws which govern the thought-process are not identical with the laws of image association.

Binet's position is not so precise and clear-cut. He too finds his 'thought themes' inexplicable by the laws of automatic association. He notes the presence of concrete thoughts without imagery, also cases where the imagery is illustrative of but a small portion of the phenomenon. Often the image is not even coherent with the thought; one thinks one thing and images another. But on the side of interpretation, he is apparently not entirely convinced that his concrete thoughts are, as such, elemental. In his first work (5) he brings out the 'intention' as an explanatory category for them, that is, he emphasises the dynamic or functional phase of thought without clearly positing a static element. In a later work with Simon (6)

he examines the consciousness of certain aphasics and imbeciles, and concludes the presence of imageless thought because of the evident absence of verbal imagery in his subjects. He dismisses the possibility of other forms of imagery as constituting the real content of their thought processes, because, he says, it is impossible to imagine acts. He appears to have overlooked the manifold possibilities of kinesthesia in this connection (cf. Colvin (14)). His conclusion is that an intellectual *sentiment* or feeling gives us an advance taste of our thought; we thus perceive it before it becomes definite. In his discussion of current psychology for 1909 (8) he gives still another turn to his interpretation of the issue by identifying his 'intention' with 'attitude,' or 'tendencies of action which are arrested.' "The fundamental fact is the motor tendency." When this is inhibited we have the sense of attitude which is "probably but the psychic expression of corporeal attitudes which seize upon or are produced in us in a purely cerebral manner without leading to completely realised muscular contractions."

In turning to Woodworth, we shall not find it necessary to trace in detail the methods and results of the experiments noted in his four publications on this subject. These are all sufficiently well known to American readers. There is no question as to where he stands regarding imageless thought contents and their differentiation from images, but his work has possibly suffered for lack of more systematic treatment. The only other work on this subject, aside from Thorndike's utterances (39), which has issued from Columbia, namely, that of Betts (4), has been rather severely criticised (cf. Fernald (17)), and I fear justly, because of the author's failure to take into account the presence of verbal imagery in his subjects' reports. The experiments were performed on a large number of persons, and the lack of exact control is manifest in them.

Coming again to Woodworth, he as well as Bühler has focused attention on the adequacy of the imagery as a criterion for its essential connection with thought (44, 382 ff.). This point is criticized vigorously by Rowe in his recent study of voluntary movement (34). Rowe's first point is that with Bühler ruling out the image as being inadequate for thought, and Woodworth ruling it out as a cause of movement we have a *reductio ad absurdum*, since it is only necessary to carry the matter a step ^{further} to be convinced that images are not essential to any of our mental processes. Rowe contends, second, that many things come in to modify the importance of the adequacy or inadequacy of the image in causing movement. Reflexes, instincts,

habits and the inflowing sensations are of vast importance, and need not at all be included in the image. Third, he argues that this idea of adequacy is not a psychological but a logical concept. It is a mere assumption that imagery to be adequate must contain what will be contained in the forthcoming movement.

Regarding the first point, it appears that the *reductio ad absurdum* is precisely what is indicated if one accepts the results achieved in favor of imageless thought. No one, I think, would quite wish to carry it to this extreme at once. That would be taking a rather broad jump at the conclusion. But Woodworth has spoken of imagery, generally, as 'by-play,' and in a specific instance as having "more the appearance of sparks struck off by thought in its progress than of thought itself" (45). The last figure of speech I find I have clearly imitated, though unconsciously, in my own conclusions on a specific case of introspected meaning (29). Even Pillsbury (32), discussing the topic theoretically, decides that imagery is subordinate to meaning, although he does not describe what the meaning content is, psychologically, seeming rather to indicate that the psychologist when he analyzes must content himself with images and leave the meanings to a somewhat different, broader and more concrete, aspect of the conscious process.

The second point refers only to the consciousness of voluntary movement, and finds its justification for Rowe in his results, which indicate that 'resident' and 'remote' images are alike superfluous to voluntary control. The movement, he says, is initiated by perception or 'the image of its natural stimulus,' and the control, instead of being effected by imaged or imageless thought, is entirely a matter of the immediate sensations and perceptions at hand. In short, the whole reduces to a continuous succession of motor adjustments. Just what determines an act as voluntary is the fact that it is 'cognitive.' The initiation of the movement is not the important thing, but the *control* which means cognition and is a sense perception of the end. That cognition should be equated with sense perception is apparently assumed as an incontrovertible postulate. The third point indicates this. We shall defer this question until the conclusion of our summary.

Let us consider, next, two other recent investigations on volition. Ach in his recent volume (2) gives as the result of his analysis of the act of will the following phenomenal content. The act consists in four 'moments': (1) the '*anschauliche*' moment (sensations of strain), (2) the '*gegenständliche*' moment (idea of aim and relation, means and ends), (3) the '*aktuelle*' moment (the activity '*ich will wirklich*'),

(4) the 'zuständliche' moment (*Bewusstseinslage* of strain). The characteristic factor is the third of these. It is a unique element, being neither 'intentional,' nor a mere knowing about (*Bewusstheit*). No image factors are essential to it, though it is but a moment in a complex from which it can never be absolutely freed. What characterizes it as the essential feature of will is the fact that in its absence, although the other three moments may still be there, the character of volition is gone from the experience.

Michotte and Prüm (27), with a series of independent investigations in which a somewhat different method was employed, arrive at conclusions which substantiate Ach's main contention, namely, that a unique 'consciousness of activity' is essential to the mind in willing. For them, too, it is not a feeling of muscular activity, and is incapable of separate existence, being described, rather, as a simple 'modality' affecting certain phenomena so as to characterize them. Both investigators refer it to the *self*, and find in it an *actual*, not an *intentional* reference.

In reading these reports in connection with the work of Rowe, one is struck by the immense richness of content in the former, and the rather schematic poverty of the latter. Rowe has given us some interesting tests on the mechanics of volition, the real problem of its psychology he has missed entirely. And yet, he would probably retort that it is just the psychology which he has given, the rest with which Ach and Michotte deal is not psychology but philosophy. So here again we meet with the central question of all this work as to what psychology may be permitted to find in consciousness.

Let us deal next with a series of experiments on abstraction which have adduced important results bearing on imageless thought. First Külpe (23), in a report before the first German Congress for Experimental Psychology (1904), gave the outcome of some tentative investigations which date back to 1901. As a result he notes that the process of abstraction is based on apprehension and not on sensational differences, and he concludes from this that a distinction must be made between sensations and our perception of them: "that this distinction must be made in much the same sense in which we distinguish between physical phenomena and our consciousness of them; that, in other words, the old doctrine of an inner sense with the involved idea of a distinction between the reality of consciousness and objectivity must now have its opportune renewal in the domain of psychology."¹ Thus are we brought into close contact with the 'act and content' psychology of the Austrian School.

¹ Quoted from Moore, 28, 92.

Grünbaum (18), working under Külpe's direction, brings renewed proof of this 'inner sense' in terms of the relation of sameness, a 'meaning moment' which his experiments reveal as an independent act which may be experienced without either member of the relation being apperceived. Furthermore, he finds this experience to be differentiated from the character of the members between which it operates, inasmuch as the same relation may exist with quite different relational bases.

Moore's work (28), which has recently appeared, was conducted partly at Leipzig and partly at the University of California. He too, substantiates the results of Külpe and Grünbaum. Perception, he describes as a "process of assimilating the data of sense experience to their appropriate mental categories." These mental categories are treated as the result of past experience presented as a supplement to the sensory data, but not reducible to either sensation, image or feeling. The principal criterion which Moore advances for the independence of this factor as an imageless thought or meaning, is contained in the act that any degree of certainty of recognition may accompany any degree of perfection in the percept. One may, perhaps, doubt if perfection in the percept is adequate to his contention. The test which he makes is that of ability to reproduce the figure which was recognized as having been repeated. This, to my mind, only substantiates a view which I have recently expressed (29), namely, that knowing and expressing are distinct processes. But this does not of itself prove that the knowing process is imageless. Many would contend that the physiological 'set' which is aroused by the perception is adequate to a judgment of certainty or uncertainty regarding recognition, without the exact contents of the percept being at once reproducible.¹

The negative phase of Moore's criterion has more weight, in my estimation: the fact that his subjects at times knew all about the figure and were able to draw it from memory, yet were doubtful as to its being the common element of the series. But even here there is a possibility of confusing the content of these concepts with their functional value. So long as relational factors remain 'embedded,' to use Miss Calkins's term, in what are clearly sense perceptions, we shall find it difficult, I fear, to convince the opposition of their elemental character.

¹Cf. in this connection an illustration by G. E. MÜLLER: Zur Analyse der Gedächtnistätigkeit und des Vorstellungsverlaufes. *Zsch. f. Psychol.* Ergbd. 5, 1911, pp. 139-141.

Passing over without comment, as we must for lack of space, the important contributions of Bovet, first in substantiation of the general results obtained at Würzburg (10), and more recently, in contributing to the differentiation of the attitudes involved in, the sense of duty, or oughtness (11), we must proceed finally to a consideration of the work of Titchener and his school. Titchener has been the one American psychologist who has given most attention to the phase of experimental psychology which we are reviewing. Not only has he expressed his views at length in a series of lectures (40), but he has also directed a number of his students in experimentation with the Würzburg method.

Titchener's position, as is well known, is negative. He confesses to the 'sensationalistic bias.' In putting ourselves in touch with the reports from his laboratory, we shall do well to understand first the nature of his antagonism to the new thought element. Being of a richly imaginative type, he does not find himself among the 'tailless foxes' of Winch's fable (43). Neither does he see any reason for granting the existence of foxes which have not at least a vestigial caudal appendage. Consequently, the prime question for him is: *do images mean?* If they do, why accept a new element for meaning? In substantiating his conviction that images are the only meanings which psychology can adduce, he declares that meaning must be taken *sub specie existentiae*; all else is logic. It is the confusion of logic with psychology which brings Bühler to the statement that "it is impossible to ideate a meaning; we can only know it." The natural consequence of this confusion is, for Titchener, the 'act and content' doctrine of Brentano and the Austrian School, which separates the act of knowing from the content known. This doctrine he has accordingly subjected to a somewhat lengthy destructive argument. Regarding this doctrine as the natural consequence of imageless thought, if I may be permitted to refer again to my own distinction of the knowing and expressing phases of consciousness, I should like to submit that it is not impossible to conceive thought and knowing from the point of view of mental content, as existing in direct causal relationship with the expressive consequents which are sensational and imaginal. But Titchener would doubtless contend that in this case the antecedents are of the same quality as the consequents. This, however, is the point at issue, and dogmatic assertion on either side will never settle the outcome. We must turn, then, to Titchener's criticism of the findings of imageless thought. The findings, he says, are not convincing, because the observers were all under the

influence of the 'stimulus-error' in their introspective reports, that is, they have reported *about* their experiences, without really describing the contents which they experienced. This is much the same criticism which had previously been uttered by Dürr (15) and von Aster (3).

Now, it has not been denied that '*Kundgabe*' is mixed with all introspective reports of this nature. But if the error be regarded so serious as to discredit the entire result of Bühler's investigation, as Titchener appears to believe, may we not carry the point a step farther and deny the value of *all* introspection? Indeed, in a recent discussion among psychologists, this position was vigorously maintained by two among those present. We cannot go into this question here, but assuming that introspection possesses at least a relative value, as I believe would be commonly accepted, I am impressed by a counter-error in some reports on the higher mental processes which is equally insidious to that of *Kundgabe*. At least the latter, if the observer be reliable, can be counted on to *intimate* what was going on in the mind at the time of the experiment. The other error to which I refer confines introspection to the known mental categories of sensation, image and feeling in which the observers have been schooled.

A colleague in a neighboring institution who is conducting some investigations on the learning process, reported recently that in his own laboratory, with relatively untrained subjects, imageless thoughts were plentiful in the introspective reports. When he transferred his work to one of our leading laboratories and repeated the experiment with 'trained' observers, nothing was reported but sensations, images and feelings. At first glance this would seem to indicate that the untrained observers had failed to analyze their experiences, and this may well have been the case in a certain degree. But it may also indicate a predisposition on the part of the 'trained' observer which is decidedly prejudicial to the discovery of *meanings* in experience. I seem to detect this predisposition in all the introspections which the Cornell investigators have put forth.

Pyle in his study of expectation (33) concludes that this conscious attitude consists in organic and kinesthetic sensations depending on a psychophysical 'set.' No 'knowing' factor is registered by his observers, and the difference between definite and indefinite expectation is regarded as entirely a matter of the 'situation'; it is not *known* at the time it is experienced.

Okabe in his study of belief (30) gives us results which are simi-

larly external to actual meaning. He finds, it appears, no difference between the consciousness of belief and the consciousness of disbelief. Both are 'consciousnesses of the same kind,' consisting in sensations and images, and occasionally, though not necessarily, emotional. One might imagine that Okabe's results would apply equally well to a description of the esthetic attitude, the ethical attitude, the consciousness of understanding, or indeed any other of the higher apperceptive states of mind.

His improvement of the method of introspection to the extent of confronting each of his subjects at the end of the investigation with a tabulation of his utterances, does not seem to have accomplished more than to elicit from each observer the assurance that the tabulation was correct. Had alterations been suggested here and there, the improvement might have proven its worth; as it happened, it appears to have been quite a perfunctory affair. This same procedure was followed by Clarke in her work on conscious attitudes (13), and with the same equivocal result.

Clarke's analyses bring clearly into focus the point which I have been making. Her subjects lay all stress on the sensory factors in their experiences, and whenever meaning factors appear, they are either passed over by the author, or referred to as *Kundgabe*. It is interesting to note, however, that in the analyses given of some sixty attitudes such as approval, caution, difficulty, surprise, etc., there appear several which, as named, clearly involve an element of *knowing*. For instance, the 'consciousness that the letter was too small,' 'fear that I had reacted too quickly,' 'I ought to know that,' etc. Yet each of these is unblushingly analyzed into 'muscular strain and organic sensation,' 'slight sinking of the stomach and diaphragm,' and the like. The author does not make it quite clear whether she would have us understand that an attitude such as 'I ought to know that' is really equivalent to 'organic sensation and disagreeable feeling,' or whether the *meaning* of the attitude is a transcendental factor. I find Titchener, himself, somewhat unclear on this point. If, as he says, meaning is always context, and context is presented as a group of sensations and images, this would seem to imply a very elaborate system of symbols in terms of imagery and kinesthesia to fill all the demands of the knowing attitudes. The difficulties met by the James-Lange theory of emotion in equating organic complexes with diverse affective experiences,¹ suggests the far greater difficulty which must

¹ For a brief statement of these difficulties, cf. TITCHENER, E. B. *A Text-Book of Psychology*. New York, 1910, p. 477.

arise in equating the manifold possibilities of thought with groups of sensations and images. The one thing which introspection has revealed with some degree of certainty, is the fact that the same thought or relational experience may be present in the same as well as in different minds, with quite variable sensational and imaginal accompaniments.

The other alternative is to shift meaning to the realm of the transcendental or to the unconscious, and I submit that either course is simply to avoid the issue. The consciousness of meaning is a consciousness which we all experience; whether it can or cannot be analyzed is another matter. But the problem is not materially helped, so far as I can see, by denying its psychological existence.

It is evidently under the assumption that images may carry thought, without *being* it, that Clarke concludes from her experiments that the imagery need not be specific or elaborate for this purpose. As for inadequacy of the image, we have, she says, no criterion to tell us how "clear or complete an image must be in order to carry a meaning." In repeating experiments with Woodworth's 'rule of three' method, she finds "not a single case of a relation being consciously carried in non-sensory terms." And yet, I have a feeling that the following experiment, at least, would have been interpreted somewhat differently by Woodworth: "Red : blue :: green : yellow. I started to say this automatically. Then I repeated the stimulus and said 'intermediate' verbally. *Some kind of consciousness that meant 'principal colors.'* I did not say 'principal.'"¹

Thus we see that it is the fundamental problem concerning the psychology of meaning which directs as a powerful determining tendency the outcome of all these experiments on the content of thought. Those of us who believe that meaning is a conscious factor, directly given in our experience, find it unequivocally described in our introspective data, despite all the errors of *Kundgabe* which may creep in. Those, on the other hand, who believe that meaning is a logical concept which can be psychologically observed only in terms of sensory 'vehicles,' obliterate the *Kundgabe* from their reports and direct attention on the sensations and images which may be present. The experiments of the latter group have been chiefly negative, that is, they fail, as of course they must fail under such an 'Aufgabe,' to find the imageless content. But positively, I cannot see that they give much promise of being able to work out a psychology of thought in terms of sensory symbols.

¹ L. c., p. 243. The italics are mine.

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PSYCHOPHYSICAL MEASUREMENT METHODS

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The activity in this field of work within the last two years contrasts curiously with the indifference with which the problems of psychophysics were treated formerly. The number of papers mentioned in this report is considerably greater than that of the papers enumerated in the PSYCHOLOGICAL INDEX for the preceding years. It is convenient to begin the presentation with the new edition of Wundt's *Principles of Physiological Psychology* (10). The chapter on psychophysics was considerably enlarged and thoroughly overhauled, but the characteristic features of the views advanced in the previous editions are unmistakable. The view that psychophysics is not an independent science, but only the helpmate of psychology, is strongly accentuated. Psychological observations made incidentally to psychophysical experiments are sometimes of greater importance than the direct outcome of the experimentation. Wundt's criticism of the values derived from the statistical treatment of long series of experiments or of experiments on large groups of individuals are of great interest. The values obtained in psychophysical experiments differ from those derived from physical measurement also in this, that their variations are not due to accidental errors but to variations of the quantity determined. By combining results from different periods of time or from different stages of training one is liable to obliterate the traces of factors which are of the greatest importance, and to obtain results void of psychological significance. The presentation of the methods of minimal changes, of mean gradation and of right and wrong cases has not been changed materially, but the chapter on the methods of counting off contains some discussions about the curves of distribution, a short criticism of the method of ideal ranges (*Idealgebiete*), and a presentation of the method of multiple cases, for which the material of Keller's acumetric experiments is used.

William Brown (1) published a study on the use of the theory of correlation in psychology, which is all the more significant since it carries with it the authority of Pearson. In the first part the correlation coefficient, the correlation ratio, the probable errors, and multiple correlation are explained. The author then proceeds to discuss other methods of determining correlation and mentions

Pearson's criticism of Spearman's work. Then follows an outline of the history of the theory of correlation as applied to psychology. The last part contains the description of a series of tests which were made on fairly large and homogeneous groups of students. The tests comprised crossing out letters, bisecting and trisecting lines, the speed of addition, the Müller-Lyer and the vertical-horizontal illusion, memory, combination, association and general intelligence, marks for drawing and general school marks. The correlation is low and the groups of coefficients show no hierarchical arrangement, which to some extent bears out Thorndike's results. Spearman's formulæ are found not to give satisfactory results.

The paper mentioned under (2) is a reprint of the last chapter of the author's doctor's thesis.

The third paper of William Brown (3) contains the study of one of the more complicated mental functions. A group of 83 students was examined in geometry, algebra and arithmetic. The papers were first marked according to the ordinary school standard, and then according to a differential system of marking based on the analysis of the mental processes involved in answering the questions. The correlation between algebra and arithmetic is greatest, then come geometry and arithmetic and last geometry and algebra. The author infers that geometrical ability is only related to algebraical ability through the mediation of arithmetical ability. The result that memory of preceding propositions is most closely related to the other forms of geometrical ability, is a similarly interesting result. The author proposes to test in a future publication the existence of a general mathematical ability or a general geometrical ability. For this investigation new formulæ will have to be devised, since those in existence are open to criticism.

Warner Brown (4) took up the analysis of the notion of the threshold of difference along the lines pointed out by Peirce and Jastrow. On the basis of 75,100 experiments on lifted weights he finds that all differences, no matter how large they are, are misjudged a certain number of times if a sufficient number of experiments is made, and all differences, no matter how small they are, are judged correctly in a certain number of cases, the percentage of right judgments increasing when the difference between the stimuli increases. From this he concludes that there does not exist a threshold of difference, if this word designates a difference which is always judged correctly, nor has it any meaning to speak of the threshold as of a difference which is so small that it

always passes unnoticed. There remains the possibility of defining the threshold in terms of the relative frequency of correct judgments, but Brown does not see any reason why one value should be preferred to any other. The author certainly has the merit of expounding Peirce and Jastrow's ideas and of illustrating them by means of a large experimental material. A number of investigators, however, think that this criticism of the notion of a threshold is rather out of date. A distinct addition to our knowledge is Brown's report on an investigation, as to whether the form in which the judgment is expressed influences this judgment itself. He made series of experiments in which all the conditions, except the way in which the judgments was expressed, remained constant and showed that the relative frequencies of correct judgments in series with oral expression of the judgment and in those with motor expression were not the same. These differences are attributed to some bias or mental defect of the subject.¹

It becomes acknowledged more and more that an original treatment of the problems of psychophysics cannot be given without an adequate knowledge of certain branches of mathematics, although the practical applications require only very little mathematical ability. The theorems, which are needed in psychophysics, are not given in most of the text-books and the original treatises are not easily accessible to psychologists. The second edition of Czuber's well-known book on the calculus of probabilities (5) conveniently supplies this need and will be found a great help to the student. The following chapters will be of special interest to psychologists: The series of Bruns for the measurement of groups, four place tables of the probability integral and of its derivatives, and a short presentation of Pearson's views. Bruns's theory of the measurement of groups has found only little attention, partly because the author spoke in his book as a mathematician to mathematicians. It is to be hoped that Czuber's clear presentation will do away with this difficulty and that the value of Bruns's series for the problems of psychophysics may get a fair test.

The present writer (6) published a study of the method of multiple cases. The distinctive feature of this method is the admission of five different judgments. The notions which served for the analysis of the data of experiments with three judgments are sufficient also for the analysis of this method. The notions of the probability of a judgment and of the psychometric functions are fundamental.

¹ See Special Review on p. 218.

First a method is given by which the observed relative frequencies of the five judgments can be adjusted and it is seen that the calculated and the observed values coincide remarkably well. Then the theory of ideal ranges is taken up and the conditions are found under which these qualities may be used as measures of sensitivity. The results of Keller's acumetric experiments show a rather close agreement between the interval of uncertainty and the ideal range of the middle judgments, so that one has to conclude that the above-mentioned condition is fulfilled. It remains to be seen whether experiments with other stimuli will give similar results. The last part of the paper contains a criticism of a method for the determination of the sensitivity, which was suggested by Ebbinghaus. The results obtained by this method are in no relation to the values which are found for the interval of uncertainty.

A short presentation of the theory of psychometric functions was given in the paper on the method of constant stimuli (7). One has to distinguish between the treatment of the results by indifferent formulæ (such as Lagrange's or Newton's formulæ of interpolation) and the adjustment according to a definite hypothesis on the psychometric functions. Each procedure has its own advantages. We must be guided by experience in the choice of a definite hypothesis on the psychometric functions and find out which hypothesis fits the given data best. Two such hypotheses are tested and it is shown in detail how the relative value of an hypothesis must be judged.

Keller published some time ago the results of a very extended series of acumetric experiments without giving an adequate discussion of his material. The results of an adjustment of these data according to the so-called $\Phi(\gamma)$ -hypothesis was given by the present writer (8). It may be of general interest to psychologists, that the time spent in working out the data is very small when compared with the amount of time necessary for obtaining them, provided that the work is done systematically. The proper treatment of good experimental data requires less mathematical ability and is less laborious than is commonly believed.

Wirth's paper (9) on the direct treatment of the data of psychophysical experiments is one of the most significant productions of the last year. It corresponds to the treatment of the psychometric functions by an indifferent hypothesis and has a very wide range of application. The salient feature of the paper is the restriction to a range outside of which one of the psychometric functions of the extreme judgments has the value *one*. This is a new idea, because

until now the psychometric functions of the extreme judgments were always represented as asymptotically approaching the values *zero* and *one*. Starting from this idea the author develops formulæ for the threshold, for its mean variation and for the mean square of the deviation. Some of these expressions are rather complicated, but Wirth gives formulæ of approximation, by which the calculation requires an insignificant amount of work. The usefulness of this article is increased by numerous examples, with which the application of the formulæ is illustrated. There is no doubt that Wirth's ideas will prove useful also in other fields of work. The realm of psychophysical measurement methods to-day is not as restricted as it was in Fechner's time. One may say that it reaches as far as the method of experimental variation of the conditions. In some fields of work, as for example in the investigation of higher mental processes, it is impossible to collect a very extended experimental material and it is exactly in these cases that Wirth's method will prove useful.

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REPORTS OF MEETINGS

EIGHTH ANNUAL MEETING OF EXPERIMENTAL PSYCHOLOGISTS

Experimental psychologists from Clark, Dartmouth, Harvard, Hobart, Johns Hopkins, Mount Holyoke, Princeton, Tennessee, Vineland, Wesleyan and Yale gathered for their eighth annual meeting at the Psychological Laboratory of Cornell University, Monday to Wednesday, April 17-19. Among the visitors was Professor G. Tschelpanow, of the University of Moscow, who is studying American laboratories in order to perfect plans for the erection and equipment of a new laboratory building in the Russian university which he represents.

Monday morning was spent in an inspection of the Cornell laboratories; special interest was shown in the demonstrational apparatus. The afternoon meeting opened with a paper by Bentley (Cornell) on 'Comparative Psychology and the Naturalist.' The reader pointed out the essential differences between the appreciative or sentimental attitude toward animate nature, and the scientific attitude whose purpose is accurate description and comprehension; he urged that, in order to the study of the animal consciousness, the field-observations of animal behavior made by the naturalist must be supplemented by psychological experiments, designed to show, *e. g.*, what sense-departments play a decisive rôle in the animal's conduct and how the creature solves problems presented to it by typical situations. Porter (Clark) reported an extended series of observations carried out by L. W. Sackett on the porcupine, which showed that in certain traits of intelligence this animal ranks between the raccoon and the monkey,—a result which contradicts the popular notion of the porcupine's stupidity. In the discussion aroused by these two papers, the question came up whether or not, from the point of view of university economy, it would be advisable to affiliate experimental psychology to the biological sciences. It developed that, in many universities and colleges, experimental psychology is still in a transitional period: the majority of those present believed that the subject should be placed on the same level with, but should remain independent of, the physical and biological sciences.

The topic of the evening's discussion was 'The Methods of Psychology.' In coöperation with the members of the graduate seminary, Jacobson (Cornell) had prepared in tabular form an empirical statement of psychological methods as advocated and employed in current text-books, and Foster (Cornell) had reduced this empirical list to a logical table, which he presented and explained. The subject was discussed further by Dodge (Wesleyan), Holt (Harvard), Sanford and Porter (Clark), Titchener and Whipple (Cornell).

On Tuesday morning, Dodge demonstrated by means of lantern slides the operation of a highly sensitive recording device of low latency. It is based upon the principle of photographing, upon a falling sensitive plate, the projected shadows of a lever which records highly magnified movements of vibrating forks, of membranes indicating pulse or breathing movement or change of volume, and of muscles concerned in such reflexes as the wink and knee-jerk. Ogden (Tennessee) reported experiments on images of memory and imagination, which were designed to test the results obtained by Perky. In experiments on 'imageless thought,' he measured separately the reaction-times for the appearance of meaning, and of visual, auditory and kinæsthetic images, and found that the meanings often arose as much as 0.5 sec. more quickly than the imagery. The subsequent discussion brought out a number of interesting observations with regard to the various types of kinæsthetic images, and the translation of meanings into attitudinal representations.

At the afternoon meeting, which was held in the psycho-educational laboratory, Johnston (Johns Hopkins) outlined the research work in progress at Baltimore. Experiments on the most economical method of learning in animals showed that the distribution of trials into daily groups was most effective; after an interval of 60 days scarcely anything had been forgotten. The results of Watson's experiments on white rats proved that these animals have practically no color vision. Watson's new circular maze, with various standard adjustments, was described as practically completed. Johnston's own work on the sensory equipment of dogs revealed the fact that the animals do not rely to any great extent on vision, especially in novel situations. McComas (Princeton) gave an account of tachistoscopic experiments on types of attention, in which he had found a high degree of correlation between attention-span and association-times. Angier (Yale) raised the question whether it is possible and advisable to correlate the credit-marks of college students with some

other and more objective tests of their standing and intelligence. The discussion brought up a number of practical problems, which are awaiting their solution at the hands of an applied psychology. While some of those present found in the use of correlations a promising method for determining, *e. g.*, what chances a given individual has of success in a given occupation, others expressed doubt on the ground that a few experimental tests in a restricted field could not do justice to the complex problems presented by the occupations of everyday life. The significance and applicability of the method was later demonstrated by Goddard (N. J. Training School, Vineland) in the case of 400 backward and feeble-minded children, whose intelligence he had studied by the Binet-Simon tests and had compared with the intelligence of 2000 normal children. He "obtained figures of distribution which agreed so amazingly with the curve of normal distribution that there can be no doubt that the Binet tests are marvelously accurate." His tables of comparison, relating a child's intelligence as measured by the Binet tests with its intelligence as expressed by the difference between age and school standing, furnished further evidence of the usefulness of these tests.

At an informal meeting in the evening, Porter (Clark) exhibited a novel apparatus for mapping the blind spot, and Baird (Clark) showed a laboratory stand, of cheap construction, which nevertheless permits of fine vertical adjustments.

The Wednesday session was opened by Langfeld (Harvard) with a report of association experiments, involving suppression or negative instruction, under the influence of alcohol and caffeine. With 30 c.c. of alcohol, the association-time averaged 0.1 sec. faster, the reproduction-time 0.2 to 0.5 sec. faster than the normal, while 6 gr. of caffeine reduced the association-time by 0.2 sec., and had practically no influence upon time of reproduction. Introspection revealed great individual differences of attitude towards the instruction. Similar experiments, in cases of dementia præcox and manic-depressive insanity, showed in the former a high percentage of failure to suppress, in the latter a practically normal behavior. Ford (Harvard) described a new method for obtaining judgments of time-reversal; electric shocks are given, under distraction, to the two wrists at intervals varying from 1/10 to 1/80 sec. Bingham (Dartmouth) reported the results of a statistical inquiry into the minimal cost of installing and maintaining a college laboratory.

Angier reported from the Yale laboratory experiments on visual contrast with subliminal color-stimuli, and mentioned among others a

study of æsthetic symmetry, in which the influence of bilateral motor processes appeared to be essential. Cameron (Yale), studying reversible perspective, found that eye-movements are not, as is frequently supposed, necessary to this form of optical illusion. Baird outlined the results of three researches conducted at Clark University. The first is an investigation of the relations obtaining among the various taste-qualities; the results differ materially from those of Kiesow and Oehrwall. The second is a study of the organic effects of musical enjoyment and appreciation; characteristically different curves and introspective records are obtained from two types of hearer, the passive or receptive and the intellectual or critical. The subject of the third study is the stammering child. Pneumographic records show a lack of coördination of thoracic and abdominal breathing.

Tschelpanow then addressed the audience in German, describing the status of experimental psychology in the Russian universities. The progress of the science has been impeded partly by changing political conditions, partly by the close connection of psychology with philology, and partly also by the fact that students in the universities are in fact only of collegiate rank, and still go to Western Europe to complete their psychological education. Recently a business man of Moscow, Mr. Shtchukin, well-known as a patron of the modern school of painting and owner of a large gallery of modern pictures, has given 100,000 roubles (\$50,000) for the erection of a laboratory building, and 20,000 roubles for its equipment. The new laboratory is also endowed with a library of 3000 volumes, presented as a memorial by the family of a young instructor who met with an untimely death. Tschelpanow is to be the director of this laboratory.¹ Applied psychology has of late become influential in Russia, owing to the work of Netschajeff and his collaborators in the *gymnasias* and other secondary schools, where psychological instruction has been introduced. Interest in physiological psychology is sustained by the familiar names of Bechterew and Pawlow.

Smith (Toronto) reported that the laboratory of his university is in a state of reorganization; the present year has been profitable chiefly on the side of undergraduate work. Edwards (Cornell) described his experiments on suggestion, giving particulars of method and some preliminary results. The meeting was brought to an end with Bentley's report of two studies recently conducted at Cornell. The first is concerned with the factors that determine choice under

¹A more detailed account of this address appeared in *Science*, May 26, 1911.

balanced motives. Its two aims are to get at the influences operative in autosuggestion and social suggestion, and to analyze the judgment which issues from two motives of approximately equal strength. The second study enquires into the dependence of thermal intensity upon area of stimulus. The dependence appears only when more highly tuned temperature-organs are involved, and when conductivity by the skin is enhanced; no summational effects are found either for cold or for heat.

It was decided that the ninth annual meeting should be held at Clark University.

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THE INNSBRUCK CONGRESS OF 1910

Bericht über den IV. Kongress für experimentelle Psychologie in Innsbruck vom 19 bis 22 April 1910. Von F. SCHUMANN. Leipzig: Barth, 1910. Pp. xxviii + 312.

Again the German experimentalists present a substantial volume as the product of their annual meeting. The number of communications and the summary character of many of them make any thorough résumé of the volume out of the question. The most that can be done is to select a few papers for summary. The contributions of the volume consist of four relatively long reviews of literature in different fields and thirty-five shorter abstracts of papers, with a description in a third part of the apparatus shown at the congress.

The first of the papers is by v. Monakow on the development and localization of movements in man. It is insisted that to understand movements it is necessary to analyze them into the components that are derived from different phylogenetic ages, as well as into the segmental localization, their focal localization, according to their end and occasion and their mental accompaniments and antecedents. Each of these factors must be taken into consideration in any act, and in any act each contributes in very complicated fashion. No movement can be ascribed to any one part of the cortex alone, nor to the cortex to the exclusion of the lower centers and the cord. The details of the treatment can not be gone into here, but the paper gives an excellent summary of recent work on localization of function.

The second paper by M. Geiger gives a statement of the *Einführung* theory with arguments for and against different phases of theories and possible theories. An idea of the wide range of subjects

treated may be given by the four main heads which are: A, the understanding of foreign expressive movements; B, the understanding of foreign personalities; C, the personification (*Beseelung*) of the sub-human, and D, æsthetic pleasure and *Einfühlung*. Authors are cited from Hume to the present. No particular conclusion is reached that can be stated within our limits, but the work takes an authoritative character from the author's close relations with Lipps, and is a compendious summary of opinions on the topic.

Alexander gives a summary of recent work on the functions of the vestibular apparatus. A brief history of fact and theory results in the conclusion that the Mach-Breuer theory is entirely adequate, particularly for the functions of the semicircular canals. The difficulties in the investigation of the sensory functions of the canals arise from three characteristics: (1) while the other sense organs have distinct functions, and qualities that do not pass over into each other by slight gradations, the vestibule has no function that may not also be performed by eye, or by some one of the tactual-kinæsthetic group; (2) the nerves of the labyrinth do not make direct connections with the cortex, but their activity can be known only through the reflexes that they excite; and (3) while the other senses only change the amount of stimulation in relatively slight degree in pathological conditions, the vestibular organs undergo a change that is altogether different, quantitatively and qualitatively, from the normal. It is doubtful if appreciation of the vertical may be ascribed to the vestibular apparatus, but it has an important function in aiding in the perception of the movement of different members of the body and in the appreciation of weight and resistance. This points to the tonic influence of the labyrinth as highly important. A full discussion of the reflexes excited by rotation is given, including means of distinguishing between the nystagmus and dizziness of vestibular origin and those of optical and pathological origin. Even more interesting because less well known is the discussion of the thermal, mechanical, and galvanic methods of exciting reflexes from these organs. They are important because it is possible to excite an organ on one side of the head and leave the other unaffected. Irrigating the ear with water above or below the body temperature will produce characteristic but opposed nystagmus and accompaniments. The same effects may be induced by changing the intralabyrinthine pressure. This stimulus can be applied only in a few individuals who have unusual movability of the windows or in case of labyrinth fistula. Galvanic stimulation of the ear with the kathode on the ear produces nystagmus

toward the kathode. On opening the circuit the direction of the nystagmus changes. Much less is known of the functions of the sacculus and utriculus. The reflexes that may be ascribed to them are (1) the compensatory eye-movements, (2) reflex head and body movements, and (3) the maintenance of equilibrium. It seems probable that the semicircular canals affect the tonus of the eye-muscles while the vestibular apparatus is of more influence on trunk and limbs. The functions of this part are much less well understood than are those of the semicircular canals.

The longest, and to an experimentalist probably the most useful, of the general reviews is by Ranschburg of the results of the experimental psychopathology of memory. It brings together an immense amount of material in every field and covers the ground from the theories of Freud to the most technical of the methods of diagnosis of the feeble-minded and insane. He is by no means ready to grant all of the claims of Freud and Jung as to the mechanism of forgetting or even as to the facts upon which the theories are founded. He adduces the work of Schnitzler as evidence that it is not yet assured that emotionally toned ideas will always give delayed reactions. Schnitzler's results seem to cast doubt upon each of the fundamental laws of Jung. Emotionally toned ideas do not always give slow reactions, in fact it is possible that the reaction is quickened, and there is no evidence of general delay in the post-critical reaction. He denies also that it is assured that the unpleasant tends to be forgotten. The work of Gordon and Külpe gave no evidence of an effect of feeling of any sort upon the course of retention and Kowalewsky and others have found that in almost 70 per cent. of children the disagreeable is more likely to be retained than the pleasant or the indifferent. His own studies, among others, convince him, too, that the forgetting of every-day life which Freud ascribes to the obliviscence of the disagreeable is due rather to the various forms of inhibitions that Müller and Schumann and Müller and Pilzecker established, particularly to effectual inhibition. Evidence of this is found in the similarity to others of the names that are usually forgotten, and in the fact that the wrong number that is added when numbers are mistaken is often very near the right one. In normal cases, R. insists, forgetting is never due to the disagreeableness of the memory, but is always due either to fading of impression or to associative or reproductive inhibition.

The second part of the paper summarizes all of the current methods of measuring anterograde amnesia as opposed to the more recent

studies of retrograde amnesia with the results obtained. The summary of results is too long for complete statement. It is interesting to note that Ranschburg inclines to the view that patients in the excited stage of manic-depressive insanity have rather better capacity for learning and retention than in the normal condition. Characteristic changes are found in capacity for memory for each of the forms of alienation. It is interesting also that the perseverative tendency and associative retention may be differently affected and show different changes in different diseases. The congenitally feeble-minded, *e. g.*, show good perseveration with diminished associative recall, while the feeble-minded as a result of disease (paralysis, dementia præcox, etc.) have fair associative recall with defective impressibility and perseveration. Curves are given for the different memory activities of normal, feeble-minded, and delinquent children. Perhaps the most valuable part of the work is the summary of results of tests of different forms of disease that makes possible a differential diagnosis. The comprehensiveness of the study may be indicated by the fact that the bibliography contains 177 titles. The results are well arranged for reference in the body of the paper.

Of the shorter papers most are to be or have been published in other places and only abstracts are printed in the volume. Some few of those not to appear elsewhere may be briefly summarized. Spearman presents a modification of his method for the elimination of chance errors in the determination of the coefficient of correlation. It is based upon assumptions that give it a wider validity than the old method.

Exner gives a summary of different investigations which convince him that acquired forms of response may be inherited. Chauvin induced changes in the axolotl, ordinarily a water animal, by subjecting it to suitable temperatures, that made it change to a land animal as does the related amblystoma. This peculiarity was shown by the offspring. The strength of this argument has, I believe, already been turned by showing that the amphibious character of the axolotl is the usual one and the change may have been in no way related to the experiments in question. The other instances adduced are certain changes in the egg-laying habits of species of salamander, changes in the diet of caterpillars, and changes in the leaf rolling instinct of other caterpillars. Whatever one may think of the evidence, Exner's interpretation is of interest.

W. Köhler presents a suggestive note on the possibly discontinuous character of the tone qualities. His experiments grew out of the

tendency to refer a vowel quality to certain pitches. In two subjects he found that a note of about 265 VD was similar to *u*, 528 to *o*, 1054 to *a*. The suggestion is that these sounds have the same relation to the musical scale that the primary colors have to the spectrum. In answer to questions he asserted that there was a quality lower than *u* that corresponds to the *m* sound and probably others still lower that give critical points for sensational qualities as the vowel sounds do for the upper notes.

Stumpf and Hornbostle reported on the results obtained from a study of the phonographic records of primitive music that the psychological institute at Berlin has been collecting. Professor Stumpf discusses two conclusions that have already been reached. First the tendency to equal interval scales of five and seven tones, second the widespread use of fifth and fourth relations in part songs. In the Javanese five-tone scale the notes are related as $\sqrt[5]{2}$, the tones of the seven-note Siamese scale as $\sqrt[7]{2}$. These prove that the occidental scale is not the only relation that may be used in music. Stumpf rejects Wundt's suggestion that the scale has arisen from making the differences in the blocks of the zylophone or metallophone equal, for observation of the blocks shows that they have been hollowed out in the course of tuning. It is evident that the tuning is by the ear rather than by the eye, and that the scales represent a maximum of beauty to the natives. On the problem of the harmonic relations of simultaneous tones, the author reaches the result that the intervals of the octave, fourth, and fifth at first appeared by chance among a large number of other relations and were selected and retained because they were the more pleasant on account of the close approach to the unison effect that makes the words more easily appreciated. These are only preliminary to the many results that may be expected from a careful study of phonographic records.

V. Hornbostle in the next paper suggests other ways in which polyphonic music might originate. The antiphonic parts might easily overlap, first by chance, then be repeated intentionally when the effect was found to be pleasant, or the use of falsetto that is found in primitive music might, in singing with others, give a consonant effect with the voices of others and be repeated through its agreeableness. Possibly the interval of the major second that is found among the Admiralty Islanders might have been developed through the failure of some voices to sing in unison. He also remarks on the complexity of the rhythm and of the melodic structure of primitive music.

The interest taken in the instrumental side of the congress may be indicated by the fact that there were forty new pieces of apparatus exhibited in addition to twenty-five by Zimmermann, not all of which were new.

W. B. P.

SPECIAL REVIEWS

A MANUAL OF MENTAL MEASUREMENTS

Technique de psychologie expérimentale. ED. TOULOUSE et H. PIÉRON. 2d ed. 2 vols. Paris: Doin, 1911. Pp. 303, 288.

This is a much enlarged edition of the volume published in 1904 by the same authors together with Vaschide. In its present form it constitutes a complete manual of tests for nearly all of the more usual experiments. After an introduction that describes laboratories (the Leland Stanford laboratory is taken as a type), apparatus in general and methods with sources of error, the work is divided into five parts: (1) measurements of simple sensory phenomena; (2) measurements of complex sensory phenomena (perception, attention and sensory affection); (3) measurements of phenomena of objectification (*Aussage* tests and reaction times); (4) measurements of intellectual phenomena (memory, association, and reasoning); and (5) measurements of individual differences. The only large fields that are not represented are the means of recording changes in circulation and respiration under the influence of stimuli. These are omitted explicitly because the technique has been developed for the most part in physiology and may be treated to the best advantage in that connection.

The discussion of the measurements of sensation is particularly full. Several new methods and instruments are recommended. One of these is the use of drops of water falling freely upon the skin for determining the temperature limen. The water is kept in a vessel provided with a thermometer, and an electric mixer and an automatic dropper, and the temperature may be changed at will by means of changing the current through a resistance in the vessel. A water dropper is also used to measure the limen for hearing. The drops are of constant size and fall upon an aluminum plate. The intensity is varied by changing the height. The relation of intensity of sound to height has been empirically determined by a method that is not described. The relation obtained looks improbable, but without

checking the method no definite statement can be made. For sight the use of colored solutions is recommended everywhere, and empirical directions are given for preparing them. For determining limina of brightness, use is made of light diffused through an adjustable diaphragm upon a ground glass plate. The siren is recommended for use in determining the difference limen for pitch, and for several other purposes.

In space perception the only unusual tests are those for the perception of solid figures on the skin that are taken over from the first edition. The tests of attention are restricted to measurement of the limen with signal and without and under distraction, marking letters or figures, reaction times and oscillations of attention. Less satisfactory is the list of tests on feeling which are restricted to arranging colors in order of affective value.

The instruments and methods for measurements of reaction times are described at considerable length. The chronoscope of D'Arsonval is recommended in this connection, on the ground that hundredths of a second are alone significant in mental measurements. The methods of making records of movements are also discussed at some length. This section constitutes, in fact, a good summary of method in that field for general reference.

The description of methods in memory has been very much changed since the first edition and now requires more than sixty pages. It is encyclopædic in its description of recent instruments and in the discussion of details of method, and makes several important original suggestions. Equally valuable although briefer is the chapter devoted to measurements of association.

Spearman is followed pretty closely in the discussion of the methods of determining correlations, and in this connection formulæ and tables are given for practical work.

The volume as a whole gives one a better impression of the full scope and possibility of the French experimental work than anything that has come under the notice of the reviewer. The descriptions are full, few short cuts are recommended, and the whole tone of the work is serious and the spirit that of painstaking striving toward the best experimental results attainable. It is comprehensive and even in all of its parts, and may be compared on equal terms with any similar work in any language.

It is interesting to note that the authors make definite selection from the different methods and either give only one or at least strongly recommend one when several are given. While the selection

is usually happy, in several cases it emphasizes the advantage of giving a number of methods as Whipple, for example, has done. Selection may be necessary for the tyro, but even he should be given a choice after some one method has been recommended for first trial.

W. B. P.

COLORBLINDNESS

Ein Fall von Gelbblau-Blindheit. S. ALRUTZ. Atti del V congresso internazionale di psicologia. Roma: Forzani, 1906. Pp. 217-218.

In view of the fact that there is a widespread interest in the facts of color vision and in color theories, it is peculiar that no notice has heretofore been taken in any of our psychological journals of the case of colorblindness which Professor Alrutz, of the University of Upsala, reported at the Rome Congress of Psychology. The importance of this general type of case is surely a sufficient excuse for a review at so late a date.

The subject, born in 1885, was a student at the University of Upsala. He was not aware of any change in his color sense, nor of any colorblindness in his family. He has had no difficulty in discriminating the red of berries from the green of leaves, but has had difficulty in distinguishing at a distance the blue hepatica from the green leaves, and in recognizing the yellow cross on the blue field in the Swedish flag.

In a test with the Holmgren wools, he confused the yellow-green, green-blue, and pure blue wools, with the green test skein; with the purple skein red and brown were confused, though he noted a difference in texture—the purple appeared uneven (*fleckig*); and with the red skein the purples were confused, though with the same reservations as to the homogeneity in the coloring of the reds.

An examination made with an arclight spectrum showed it to be perceived as unshortened. There was one color from the beginning of the spectrum in red, to a neutral zone, which began in yellow-green; this neutral zone he saw as grey '*sehr unbestimmt und ungesättigt*'; beyond that, one color extended to the edge of the violet, where a second neutral zone was perceived; then red to the end of the spectrum.

An accurate determination with the Hoffman spectroscopé '*à vision directe*' and with the sunlight gave the following results: The first color, which was called red, was most saturated at 655-650 mm. and most intense (*lichtstarke*) at 637 mm.; the saturation and intensity

decreased more and more toward yellow; the neutral zone lay between 570 and 562 mm., and had about the same intensity as the contiguous red on the left, and green on the right. The second color was called green, and was most intense at 507 mm., most saturated at 480 mm., and darkest in blue. A second neutral zone was found at the boundary between the blue and violet (467-464 mm.); it was still darker than the first. The third color, that of a newly advancing red, was darker, and not so homogeneous as the first red band, "aber doch etwas andersartig, als wenn etwas ausgefallen wäre, wie auf einem schwarzen Grunde."

A later determination with a Steinheils spectroscop, and a Nernst lamp, gave the following limits: Red, 790-770 mm. to 579-; neutral (white) 579-576; green 576-460; neutral (red and green) 460-450; 'red and black' 450 to beyond 400 mm.

The visual field is evidently not at all diminished. Red, yellow, and purple pass directly from white to red, and the green is constant. A dark blue passes from white, through red, into green.

An ophthalmological test showed that the colorblindness could not have resulted through any variation in the media of the eye.

Professor Alrutz is of the opinion that this case does not correspond to any case hitherto known.

FLORENCE RICHARDSON

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REPORT OF THE COMMITTEE ON TESTS

Report of the Committee of the American Psychological Association on the Standardizing of Procedure in Experimental Tests. PSYCHOLOGICAL MONOGRAPHS. Vol. XIII., No. 1, 1910. Whole No. 53. Pp. 107.

In 1906 the American Psychological Association authorized the "creation of a permanent committee of the Association, to consist of five members, which shall act as a general control committee on the subject of measurements." It was recommended that this committee "undertake two general lines of work, organizing as many subcommittees as it shall see fit, and calling to its assistance such outside help as it may desire: first, the determination of a series of group and individual tests, with reference to practical application, and second, the determination of standard experiments of a more technical character." The present publication contains the reports of three members of the committee.

W. B. Pillsbury: Determination of the Intensity of Sound.—The report discusses the methods of the fall-phonometer, the controlled tuning-fork, the telephone, the audiometer, and several other means of measuring amplitude. The first form of stimulus is found to be incalculable and of uncertain quality. The second is less objectionable. But "taken all in all the telephone is probably the most satisfactory instrument to use for sound experiments in the psychological laboratory. It can be made to give a constant sound that can be varied at will in pitch, in intensity and duration. It can be applied to the measurements of intensity in all fields, is invaluable in experiments in rhythm when combined with an interrupting apparatus and has a number of uses in subsidiary experiments. In addition, the apparatus required is less expensive than many of the pieces sold as instruments for the measurement of audition and the parts can be used in many other ways." At the same time, "it should be remarked that all of the methods make certain assumptions in the calculation of absolute values that may be in error and if in error, would serve to account for some at least of the divergence in results. Obviously a highly desirable advance in acoustics is the removal of these sources of error and the development of methods more suited to direct measurements." The use of pure tones is strongly advised.

C. E. Seashore: The Measurement of Pitch Discrimination.—The selection and standardization of apparatus are carefully considered. Emphasis is laid upon the difficulty and the necessity of avoiding secondary criteria of pitch. The committee recommends the use of two standard-forks of 435 vd. and 10 increment-forks, giving the intervals above the standard of .5, 1, 2, 3, 5, 8, 12, 17, 23 and 30 vd. The forks are to be actuated mechanically and then placed for 1 sec. before a selective resonator to be heard and compared by the observer. Although the constant and variable errors arising from the apparatus, the experimenter and the observer appear to be quite as numerous and varied as those in Pillsbury's tests, they undoubtedly submit much more readily to calculation or to elimination. It is a noteworthy fact that Seashore and his collaborators should have found it feasible as well as profitable to reject elaborate apparatus and to reduce the material equipment to relatively simple means. The author has wisely sounded a note of warning against hasty inference regarding musical ability drawn from the test of pitch-discrimination. Psychology is still a long way from definite practical prescription in even so elementary a function as the judgment of tonal pitch.

James R. Angell: Determination of Mental Imagery.—Current

methods are rubricated under objective (memory, distraction, 'helps,' and Kraepelin's word-lists) and subjective-objective methods. The latter, which are very numerous, rely both upon introspection and upon the character of the test-material. Purely objective methods are thrown out as unreliable. The methods of the second class are of unequal value. In making his recommendations, Angell insists that the aims of tests for imagery are many and diverse, and that, in a given case, a determination must be fitted to its own particular purpose,—whether that purpose be classification under 'types,' the investigation of school-children, the range of imaginal forms, the preferred form, or the relation of the image to mental function. "No one of these aims can be realized by any single quickly executed test and one must squarely face the alternative of abandoning any pretense of accurate information concerning the imagery of a given individual, or else of undertaking a rather extended group of tests, whose outcome may be finally problematic, if the subject proves too deficient in introspective powers." Without denying the reality of 'types,' the author warns the experimenter that "the shifting and substituting of one form of imagery for another under slight changes of conditions, where no one could possibly have suspected the occurrence of such a transformation, speedily render the unprejudiced observer skeptical of all rigid divisions into types of the familiar kind." Two groups of tests are recommended: one short group "designed to furnish a *brief* survey of the imagery capacities of the subject," and another to offer "a more *intensive* and more *accurate* analysis." The first includes the questionnaire, the test on melodies, the 'description,' spelling, reasoning and writing tests. Save for the first, these "need not occupy over two hours." The longer series of tests recommended covers voluntary and spontaneous imagination, memory of the various sense-modalities, reasoning, and voluntary control, with specific instructions given under each heading. The author is sanguine that "such a group of tests as this intelligently applied, with the minor variations which may be required by the peculiarities of a given subject, will result not only in affording a highly satisfactory inventory of the individual's equipment of imagery, it will also convey a very significant impression of the actual distribution of his imagery and of the manner in which he uses it."

The reports show with what seriousness the committee has taken its instructions from the Association. The members have set about the reduction to a common denominator of dozens of half-related methods, at present scattered through the literature. The problems

covered by their reports are now in commission for profitable work by psychologists at large. In the reviewer's opinion, the scientific value of their labors rests less upon the formulation of 'tests' than upon the elucidation of a number of matters of pure psychology.

MADISON BENTLEY

CORNELL UNIVERSITY

JUDGMENTS OF DIFFERENCE AND MAGNITUDE

The Judgment of Difference with Special Reference to the Doctrine of the Threshold in the Case of Lifted Weights. WARNER BROWN. Berkeley: The University Press, 1910. Pp. 71.

The author made on a faithful and devoted observer 75,100 experiments on lifted weights and uses the results obtained as the basis for his discussions. The weights used were modified Cattell weights. The space error was not eliminated but kept constant. The size of the hand movement was not kept constant, but the time of the movement was regulated. A detailed table shows the percentages of correct judgments for all the stimuli used. It is seen that all the stimuli are misjudged at times and perceived correctly at other times, while the percentages of correct judgments increase with every increase of the amount of difference. There is no stimulus which is judged correctly in all the cases although some of the differences used are as large as 18 per cent. of the standard stimulus. If one tries to explain the wrong judgments on large differences by a temporary slip of attention, the question arises from which point on a mistake may be supposed to be due to such a slip. One then has to define this point in terms of the relative frequency of correct judgments and the author sees no reason why one point should be preferred to any other. There does not exist, therefore, a threshold of difference and whenever one was observed, it was due to some fault in the experimental arrangement.

The author requires that the judgments between which the observer has to choose should be mutually exclusive and that their significance should be independent of differences in the meaning which the observer might attach to them. The equality judgments do not fulfil these requirements and Brown believes that the observer should always be required to state which one of the stimuli seems to be greater. To test this view a series of experiments with very small differences was made and it was found that the cases where the impression of positive equality arises are extremely rare.

The most interesting part of the monograph is the description of an investigation whether the form in which a judgment is expressed has an influence on the judgment. Three series of experiments were made. In the first series the observer had to designate the heavier weight by pushing it forward with the hand. In the second the question had to be answered orally whether the second weight was heavier, and in the third the observer had to say whether the weights were equal or not. The standard weight was 100 gr. and the comparison weights ran in steps of 2 gr. from 82 to 92 gr., in steps of 1 gr. from 92 to 100 gr., and in steps of 2 gr. from 100 to 108 gr., making the number of comparison weights equal to 19. The number of judgments 'second weight heavier' was equal to 2,068 in the series with motor expression of the judgment and 1,952 in the oral series, the total number of experiments being in each case 3,800. From this it follows that the relative frequencies of the judgments in the two series are not the same, and that the judgment depends not only on the difference of the stimuli but also on the form in which the judgment is expressed. The author thinks that this is due to a bias or mental defect of the observer, an expression which is rather unfortunate.

Weber's Law is tested by observing the relative frequencies of correct judgments for the same percentual differences and different standard weights. If the law holds good, equal percentual differences should give equal relative frequencies of the correct judgments. It is found that this is not the case, since the graphs plotted for the different standard stimuli are almost parallel. The author concludes from this result that Weber's Law does not hold good for stimuli of the range under consideration.

Finally the influence of progressive practice and the variability of the judgments are investigated. The variations of the relative frequencies of correct judgments are very small in the series with large differences and there is almost no influence at all in the series with minimal differences. There exists, therefore, no influence of practice in these experiments although one might have expected to find one, since the observer was notified of every mistake. The variability of the judgments is investigated by the median values and the averages of the mean variations of the frequencies of the correct judgments. This part of the book is unsatisfactory on account of insufficient treatment of the data.

F. M. URBAN

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The Perceptual Basis for Judgments of Extent. H. L. HOLLINGWORTH. J. of Phil., Psychol., etc., 6, 1909. Pp. 623-626.

The author uses results, which he has published in a previous treatise, for arguing against the view that the comparison of length of movement is made through the comparison of the duration of sensations arising from the movement. He favors the view that the perception of any one characteristic of a movement is just as primitive as that of any other and that the judgment of extent seems to be based on a system of signs which have been learned to mean extent directly.

F. M. URBAN

UNIVERSITY OF PENNSYLVANIA

The Central Tendency of Judgment. H. L. HOLLINGWORTH. J. of Phil., Psychol., etc., 7, 1910. Pp. 461-469.

An indifference point is found in every series of magnitudes with which we work at approximately the midpoint of the scale. This phenomenon is explained by a law of central tendency, which makes our judgments on stimuli of a given range shift towards the median value of the series. This law is tested by experiments on the comparison of the size of squares, in which the influence of motor habit was excluded so that the errors were due to the judgment of visual magnitude alone. The central tendency of judgment reveals itself underneath the positive constant error which is present in experiments of this kind. The author finds that this central tendency can not be explained by any one of the factors usually introduced to explain the indifference point, but must be explained in its own terms.

F. M. URBAN

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- WALLIN, J. E. W. *Spelling Efficiency in Relation to Age, Grade and Sex, and the Question of Transfer*. (Educational Psychology Monographs.) Baltimore: Warwick & York, 1911. Pp. viii+91.
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NOTES AND NEWS

PROFESSOR ZACCARIA TREVES, known for his studies on the psychology of work and fatigue, died April 28, at Milan, where he was director of the civic laboratory of experimental pedagogy.

PROFESSOR J. R. ANGELL, formerly dean of the senior colleges of the University of Chicago, has been appointed dean of the faculties of arts, literature and science, to succeed George E. Vincent, recently elected to the presidency of the University of Minnesota.

DR. J. F. SHEPARD, of the University of Michigan, has been advanced from an instructorship to an assistant professorship of psychology.

JOHN B. WATSON, professor of psychology at Johns Hopkins University, is to be one of the visiting instructors at the University of Chicago during the summer quarter.

THE present number of the BULLETIN, dealing especially with experimental psychology, has been prepared under the editorial care of Professor W. B. Pillsbury.

THE following items are taken from the press:

PROFESSOR G. M. WHIPPLE, of Cornell University, has been elected a member of the board of the Journal of Criminal Law and Criminology.

DR. BRUCE R. PAYNE, professor of educational psychology in the University of Virginia, has been appointed president of the George Peabody College for Teachers at Nashville.

PROFESSOR ANNA J. McKEAG, of Wellesley College, will conduct courses in education and educational psychology at the Summer School for Teachers, Mt. Gretna, Pa.

PUBLISHER'S NOTE.—The *Psychological Review Company* has been incorporated by the editors of the Review Publications. Howard C. Warren is President of the new company, James R. Angell, Vice-President, and John B. Watson, Treasurer. The Board of Directors include the officers and Arthur H. Pierce and H. C. McComas. The new company has purchased the entire business of the Review Publishing Company and will hereafter publish the PSYCHOLOGICAL REVIEW, BULLETIN, MONOGRAPHS, and INDEX.

